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This paper examines the problem of demarcating genuine science from 'pseudo-science'. It is shown that it is possible to turn the demarcation arguments which have been used against 'pseudo-sciences', such as parapsychology, against the fraud hypothesis — which is the principal normal counter-explanation for the parapsychological evidence. It is argued that the fraud hypothesis fails to be scientific on the grounds of replication, metaphysical bias, falsifiability and lack of theory. Since fraud is accepted and parapsychology rejected, the role of demarcation criteria in determining acceptable science is challenged. An alternative account of their role is presented. It is argued that the rejection of parapsychology rests on cultural differences which demarcation criteria serve to legitimate.

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Trevor J. Pinch

The problem I address in this paper can be illustrated by the following two passages, taken from two recent papers in the scientific literature:

- (a) Thus, we have a model:
 - (i) which fits all the statistical evidence, some of which is both highly specific and highly significant...
 - (ii) which... successfully predicts specific features...
 - (iii) which accounts for the whole significant ESP effect...

The model is to some degree 'ad hoc', but the significance levels are so overwhelming, and the consistency of the effects... is so clear, that it cannot seriously be dismissed as an after-the-fact construct based on chance irregularities. No such construct could hope to achieve such a close fit.¹

(b) The main objective was to develop a model within which such psi-like phenomena could be discussed in a logically consistent manner. ... the model displays a large number of features of observed psi effects, and it is not yet clear where the model disagrees with experimental evidence... The model makes very specific numerical predictions and thus suggests a large number of experiments.²

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These extracts, which refer to two *different* hypotheses, appear to be unexceptionable accounts of scientific work (apart, of course, from the mention of 'ESP' and 'psi', which is enough to raise our suspicions). Activities are described which are usually associated with science — such as, for example, explication, mathematical model building, matching observations to a model, and generating further predictions. Despite their shared characteristics, however, only one of these hypotheses — that referred to in (a) — has gained acceptability. The model described by the authors of (a) is a series of fraudulent manipulations which, it is claimed, were carried out by the chief experimenter in a famous ESP test. The model referred to in (b) is the ESP hypothesis itself — which has, by and large, little currency in science today.

The problem being raised here can be briefly stated. What are the characteristics of these hypotheses (or of their contexts) which enable us to demarcate one as part of acceptable scientific knowledge and permit us to relegate the other to the realm of 'pseudo-science'? In other words, what makes the 'fraud' hypothesis a better scientific explanation for the results of the parapsychologists than the 'paranormal' hypothesis? I hope that a discussion of this question will throw some light on the more general problem of the demarcation of science from pseudo-science.

FRAUD IN PARAPSYCHOLOGY

Few people who have given the field of parapsychology serious study would deny that *there is* a history of fraud associated with the mediums of the Victorian spiritualist era. The scientific foothold which J.B. Rhine and others attempted to establish for parapsychology in the 1930s did not prove strong enough to dissociate the field at large from its spiritualist legacy. The possibility of fraud has always haunted this area of human endeavour³ and, with the recent admission of fraud by a researcher in Rhine's own laboratory⁴ as well as the emerging consensus that the latest popular psychic wonder-worker, Uri Geller, is a charlatan,⁵ it would seem that the ghost has finally materialized.

The 'fraud hypothesis' is that certain experimental results which, it is claimed, establish some variety of telepathy, clairvoyance, psychokinesis or precognition (more generally called 'psi

phenomena'), can be better interpreted as fraud. The fraud may involve either the subject (the supposed source of the psi phenomena), the experimenter, or both acting in collusion. The difference between allegations of fraud in parapsychology and in other parts of science is that, in parapsychology, they are levelled not only at particular experiments but at the whole discipline. No-one considers that one case of fraud at the Sloan-Kettering Institute means that the whole of cancer research can be explained away,⁶ but to show that one parapsychology experiment *might* have involved fraud is, apparently, often enough to dismiss the whole enterprise.

The usual method of demonstrating that a parapsychology experiment can be interpreted in terms of fraud is to conceive of a 'normal' explanation of the results. As the aim of the parapsychologists' experiments is to show that some 'paranormal' phenomenon has occurred, the experimental design must include rigorous safeguards to prevent any 'normal' (for instance, sensory) channels of communication from being used, since these might be taken as an explanation of the psi communication. Because of these safeguards, the 'normal' explanation put forward by those alleging fraud will often be highly ingenious and sometimes bizarre, and may involve some sort of deception or conjuring trick. For example, Mark Hansel, one of the best-known advocates of the fraud hypothesis, claims that results purporting to show telepathy between the Jones cousins (a particularly successful pair of young subjects in the 1950s) were produced by one of the cousins using a dog whistle (operated by squeezing a rubber blub) concealed beneath his clothing, enabling high frequency audio signalling by the use of a simple code.⁷ Hansel alleges that this 'normal' channel remained undetected by the experimenters because they were too old to be sensitive to such high frequencies.

HOW SCIENTIFIC IS THE FRAUD HYPOTHESIS?

In the last thirty years, parapsychologists have been subjected to probing scrutiny by methodologists hoping to expose the 'mistake' which they 'must' have made.⁸ But the alternative hypothesis of fraud has, apparently, so far escaped any such exhaustive examination. One justification for this asymmetry can be found in an argument which dates back to Hume.⁹

Hume's Argument Concerning Miracles

This argument can be summarized thus: extraordinary claims (such as those made for miracles) require extraordinary proof. Because parapsychology is incompatible with the rest of science, and hence is 'extraordinary', any evidence supporting it must be at variance with all the evidence on which our present science is based. Thus the demonstration of a paranormal event must be much more rigorous than that of any event consistent with present knowledge. As the fraud, or 'normal', interpretation of the parapsychologists' results is consistent with present knowledge it does not merit such a stringent examination as the extraordinary or paranormal hypothesis.

When using the Humean argument we base our judgement of the scientific status of parapsychology not on some hypothetical independent or 'objective' yardstick, but on whether it clashes with our present scientific culture.¹⁰ To make the argument applicable it must first be established that parapsychology and science are incompatible. This claim for incompatibility has been challenged¹¹ but, even if we grant such incompatibility, the argument is irrelevant to the search for *independent* standards of rationality. We simply admit our cultural bias in favour of present knowledge, and side-step the issue of how we *know* that our present knowledge is 'scientific'. It would seem that, if the problem of demarcation of genuine scientific knowledge from spurious knowledge is to be solved, it must be approached by the delineation of characteristics of science that are independent of the content of particular knowledge claims. It may well be that *that* problem is unsolvable, and that we are forced back on our present cultural biases — but this cannot be decided a priori. It seems that some assessment of the applicability of those demarcation criteria currently used against parapsychology is needed before we embrace the Humean conclusion — that 'some men lie and cheat to deceive themselves'.¹²

The approach to the demarcation problem I follow in the present paper is to see how well those arguments concerning the scientific status of parapsychology fare when applied against the fraud hypothesis. It is rewarding to test the fraud hypothesis in this way because the development of explanations of fraud has become an embryonic science in its own right (see the opening passage of this paper). Of course, assumptions concerning the honesty of scientists are not usually made explicit, and it is widely taken as *ceteris*

paribus that scientists are honest.¹³ However, in the area of parapsychology, the task of exploring mechanisms for fraud has become a well developed pursuit — although whether it is a ‘science’ is a matter of contention.

Metal-Bending by Mini-Gellers

Before examining in detail the scientific status of fraud, I will first illustrate in more depth just how well developed this ‘fraud research programme’ has become in one area of contemporary parapsychology. This concerns the ability of young children (the ‘mini-Gellers’) to emulate the metal-bending feats of Uri Geller. My description of this area is based upon some of my own fieldwork, which consisted of participant observations and interviews with researchers and their critics. Several physicists who have flirted with the paranormal have become involved in this line of research. It will be seen that the task of producing explanations for these phenomena in terms of fraud has been pursued as vigorously as has the search for paranormal interpretations.

It has been discovered that mini-Gellers find it difficult to produce paranormal metal-bending on demand in laboratories: it is easier in their home environment. Likewise it is easier to commit fraud away from the laboratory. However, by placing the metal target inside a sealed glass container, the number of possibilities for fraudulent bending are severely reduced, and observers need not be present when the bending is attempted. But it appears that mini-Gellers, even at home, cannot bend the metal in a container without damaging the glass. In such experiments, intact tubes containing straight pieces of metal, and broken tubes with associated straight and bent pieces of metal, have been returned to the investigators, accompanied by reports of feelings of heat when the subject touched the glass, of paranormal shattering of the glass, and even of the metal bending — but, unfortunately, so violently that it broke the glass.¹⁴ These results seem to support the fraud hypothesis. Obviously children returning straight metal in intact tubes had not thought of a successful fraud, and those returning straight and bent metal and damaged tubes had damaged the tubes whilst attempting fraud. Given that it is unlikely that the children would have been so careless as to damage the tubes (thus giving away their trickery),

the advocates of fraud can simply produce the psychological sub-hypothesis that the children became frustrated at being 'caught out' by the experimenter.

However, for the parapsychologist, these results are far from an experimental refutation: on the contrary, his knowledge of psi phenomena — and, in particular, their relationship to glass — increases. Failure to bend metal through the glass indicates that glass is not completely permeable to the 'psi forces'; metal that had bent and shattered the glass had done so violently because the subject had summoned up sufficient psychic energy to penetrate the glass barrier, with a subsequent, uncontrollable 'rush' of energy. The feelings of heat and the reports of paranormal shattering indicate more of the 'shielding' properties of glass. If glass attenuates psi forces, and metal can be bent in air, perhaps subjects can bend the metal when there is a small hole in the tube? And indeed, as expected, in experimental tests the subjects' psi field can now pass unmolested through the hole, and successful metal bending is achieved. But sceptics are not satisfied, for now they claim that a small instrument could be inserted through the hole to effect fraudulent bending. And so the debate has proceeded, with scientists on each side finding the evidence to be consistent with their particular theory.

The Symmetry of Psi and Fraud Hypotheses

I have chosen this rather simple series of experiments, not because I think it provides compelling evidence for either hypothesis, but rather because it illustrates the psi/fraud schism in a contemporary field of research. Both hypotheses seem to engender equally rich research programmes in terms of experimentally testable models. (In fact, my account is by no means exhaustive, and more sophisticated experiments have since been devised.) Yet only one of these hypotheses — the paranormal hypothesis — has been subject to the cutting edge of demarcation criteria. Since the paranormal hypothesis is forced to satisfy these criteria, my argument is that we should subject the fraud hypothesis to the same sort of examination. That is to say, we should ask the question: what is the scientific status of the fraud hypothesis?

What I will attempt to show is that the fraud hypothesis can be rejected as unscientific for the same sorts of reasons that have been

used to reject parapsychology. The complete argument would review in detail the criteria proposed to demarcate parapsychology from real science. This, however, has been done in an earlier paper, in which Collins and I looked at the types of argument made for and against parapsychology.¹⁵ In order to remind the reader of the arguments against parapsychology, I will cite examples and illustrate the main themes by quotations. For rhetorical purposes I will try to make the case as convincing as possible; I hope to work in the same polemical spirit as those who have mounted the case against parapsychology (with, of course, the appropriate gloss of open-mindedness). It may be helpful to read the arguments as though a revolution in physics has taken place, and paranormal phenomena are now generally accepted, so that our problem is to expose the 'mistakes' made by those scientists who interpreted the first results of this revolution in terms of fraud.¹⁶

TESTING THE FRAUD HYPOTHESIS

Firstly, let us look at a criterion frequently invoked against parapsychology, that of repeatability. How repeatable are these claimed measurements of fraud?

Repeatability

... the only criticism not yet adequately met by present data, and the one which I contend is crucial... is the failure of ESP experimenters to produce a truly repeatable experiment — one which can be replicated in almost any laboratory as many times as desired with essentially the same results.¹⁷

It is well-known that, to be repeatable, a result must be capable of being replicated by other scientists in other laboratories.¹⁸ If we hypothesize that all paranormal results are really due to fraud, it follows that when we repeat the original paranormal experiment, we should be able to observe fraud. A replication of experimenter fraud, for instance, would require us to carry out an experiment giving significant findings which appeared to support the paranormal hypothesis (at least to the same degree as reported in the original experiment), and then to reveal the method of fraud by which the results were really obtained. Let us examine in more

detail the requirements which such a replication would have to satisfy.

The essence of the concept of fraud is that it involves illusion. Things must be made to appear other than they are. A fraudulent phenomenon must be staged so as to disguise the way in which it was really produced. In this case the fraudulently produced results must appear to support the paranormal hypothesis. Any old fraud would not do. The crucial point for replication is how we judge that this condition has been satisfied: in other words, how do we tell that the trick is a convincing one? I suggest that to ensure that the illusory quality of fraud is replicated means that the results of the replication must first be *published* as a paranormal claim, just as parapsychologists themselves have done. It is not good enough to perform an experiment that is only convincing to the experimenter; others must also judge it so. If those claiming fraud do not get their initial (apparently) paranormal result published, then the critic can say that fraud has not been unequivocally demonstrated because the paranormal interpretation of the results was unconvincing. It is as though a magician pulls a rabbit out of a hat without showing us first that the hat is empty. No replication of fraud which meets this condition has yet been reported — at least none which warrants scientific attention.¹⁹

The usual way to demonstrate fraud is to explain how experiments *might* have been produced by fraudulent means. In his notorious article in *Science*, in which he accuses several parapsychologists of fraud, Price writes:

Since I know of no evidence of this nature [the type that might convince a court] showing that X [a well-known parapsychologist] did or did not cheat, all that I am trying to do . . . is to demonstrate that X *could* have cheated if he wanted to, and that therefore we should demand better evidence than his before we believe in the supernatural.²⁰

There have been attempts to obtain experimental evidence to support such speculations. For instance, Hansel, having proposed the fraud interpretation of Soal's experiments with the Jones cousins, then went on to 'repeat' the experiments himself, thereby demonstrating the feasibility of his suggested mechanism.²¹ Similarly, Marks and Kammann have repeated some of Geller's frauds with their own subjects.²² Such 'replications' do not meet the 'strong' criterion of repeatability discussed above because a

convincing paranormal interpretation of the fraudulent mechanism was not established. The thrust of such work is therefore not towards establishing an independent replication; rather, it provides an additional challenge to the paranormal interpretation of the original experiment by showing that fraud was at least conceivable. But to emphasize the original paranormal experiment, and to produce a radically different interpretation of it, is liable to be hazardous. For instance, experimental variables which are crucial to establishing the fraud hypothesis are usually irrelevant to the paranormal hypothesis, and may not even be included in the original experimental report. Thus the clue to discovering the fraud used by one well-known parapsychologist (changing figures in the data recording) was an allegation of cheating, made by someone who had been on the fringe of the experiment; no mention of his presence could be found in the original account of the experiment.²³ The fraud explanation can depend crucially on this kind of detail. The danger is that this sort of information, if it can be obtained at all, is inevitably anecdotal and hence is easily attacked.²⁴ Unless we confine ourselves to details actually present in published accounts of experiments we seem likely to reduce the scientific debate to the consideration of hearsay.²⁵ Independent replications are the only way to make progress in this area.

One type of fraud which should be easier to replicate is fraud produced by subjects. It should be possible for the replicator to observe the subject cheating under the same ESP conditions as were contrived to obtain the original 'paranormal' results. A brief look at the literature reveals no shortage of claims of fraud committed by other scientists' subjects,²⁶ but how many scientists making such claims have observed the phenomenon themselves under laboratory conditions? There have been efforts to get subjects into the laboratory in order to repeat previously reported frauds²⁷ but, unless such efforts succeed, we will not make any progress.

The main difficulty seems to be that gifted frauds are hard to come by and are not over-cooperative in attempts to uncover their secrets. Replicators who use this excuse seem to be admitting incompetence, for competent replicators should be able to win the confidence of subjects, and persuade them that they are interested in observing paranormal phenomena — after all, it is only when a subject produces what appears to be a paranormal event that there is a chance of observing fraud. Perhaps one could use subjects who have never been tested before, and who cannot doubt the

experimenter's intentions. But, if he uses a random sample of subjects, the experimenter will probably not observe any phenomena at all, as gifted frauds are rare.²⁸ Even gifted frauds seem to be remarkably sporadic in their performances. Unless those claiming fraud can find the elusive variables which produce a strong, reliable effect, we will not have full repeatability. The difficulty in replication probably stems from the lack of adequate theory explaining the occurrence of fraud (see below).

One recent experiment has been made much of by those who favour the hypothesis of subject fraud. It was conducted by Pamplin and Collins. They observed several mini-Gellers cheating, by the simple device of looking at them from behind a one-way mirror, the presence of which was unknown to the subjects.²⁹ Were Pamplin and Collins just lucky in finding gifted subjects? They were selected via the local news media when it was known that, in the wake of Uri Geller, several young subjects with similar powers had come forward. They were therefore highly motivated to produce phenomena, especially in view of the publicity and fame accompanying such abilities. Pamplin and Collins also hit on an ingenious fraud-enhancing technique — namely, that after a period of unsuccessful bending, the experimenter in the room with the mini-Geller would turn away: this tempted them into a quick cheat, which could, however, be seen and photographed by the concealed observers. Given this simple way of enhancing the phenomenon, Pamplin and Collins were able to repeat their own observations on several occasions. However, no other scientists have as yet repeated their experiment. Indeed such repeatability may now be impossible. The success of the experiment depended both on the formation of a particular social climate conducive to finding subjects, and on the secrecy of the one-way mirror. With the passing of the Geller fad and the publicity given to the one-way mirror when the experiment was published, its repetition is unlikely.³⁰ It is too early to pass final judgment on this particular experiment but once again we appear to have an unreplicable effect.

Since repeatable evidence for fraud is lacking, it is worth investigating whether those believing in it have some metaphysical predilection for that hypothesis.

Metaphysical bias

As with religion, so with parapsychology, 'faith' is more powerful than the facts.³¹

There seems to be some basis to the claim that those who believe in fraud do have prior biases. Take, for instance, Hansel who writes: 'In my view a priori arguments determine our attitude towards an experiment, and may save time and effort in scrutizing every experiment. . . .'³² The result of such biases is clear: because the paranormal hypothesis is unlikely for 'a priori' reasons, we must expect the fraud hypothesis to be more probable even before we look at the experimental evidence for it. Price³³ and Hanlon³⁴ also both consider that metaphysical — or, as they grandly call them, 'a priori' — arguments support the fraud hypothesis. Perhaps their metaphysics has clouded their scientific judgement? There is, of course, more to science than experimental evidence, but other considerations (especially the appeal to explicitly philosophical arguments) are not of more importance than the facts. It seems that, in this case, metaphysics has acted as a substitute for solid evidence.

Falsifiability

If fresh characteristics are postulated. . . it is possible to survive almost any form of criticism. An experimental result cannot be confirmed or refuted since ESP does not operate in front of critics.³⁵

Even if the fraud hypothesis had a firm empirical base it would not necessarily meet the criteria for being scientific. After all, many pseudo-sciences make empirical claims. Scientific method demands other strictures — in particular, on the type of theorizing we engage in to explain the facts. Here the fraud hypothesis falls well short of meeting one of the basic canons. The endless task of reinterpreting data post hoc to support a particular theory seems once more to be threatening to raise its head. The logic of the fraud hypothesis entails that it can never be refuted; it is inevitably true because it mows down all empirical data. Supporters of fraud who are not ingenious enough to find a 'normal' explanation to account for the parapsychological results on the basis of subject fraud alone can 'extend the conspiracy' to include investigators and independent observers. There is always a 'normal' explanation to be found, and

such explanations are as open to imaginative innovation as science itself.³⁶ The correct explanation may not always be easy to discover. For instance, it took Scott and Haskell fifteen years before they proposed the method of fraud used in one famous series of experiments.³⁷ There is no time limit set on such essentially post hoc theorizing.

In Popper's terms, the fraud hypothesis is unfalsifiable. Unless some constraints can be placed on the types of 'normal' hypothesis which can be used, or the time permitted to find such a 'normal' channel, those claiming fraud will never be refuted. The complicated processes of deception involved in some of their explanations are reminiscent of Ptolemy's epicycles.³⁸

Theoretical inadequacy

...there is still nothing that would deserve to be called a theory... This, in the writer's opinion, constitutes the most decisive factor which casts doubt upon the 'reality' of the entire structure of parapsychology.³⁹

So far the interpretation of paranormal results in terms of fraud has been referred to as the 'fraud hypothesis'. But is it not scientifically proper that we should demand something more — namely, a theory to explain why the fraud phenomenon should occur at all? Why should a large number of scientists over a period of years be deluded by tricks, or perform such tricks themselves? There has not (to my knowledge) been a successful and agreed theory of this kind in all the time since the fraud hypothesis was first advanced. It has been suggested that financial rewards provide an incentive: this may account for the cheating of some subjects, but it does not explain why scientists are fooled — or why, if they are not, they should attempt to deceive their fellow scientists. Hansel suggests that academic rewards might also motivate some scientists,⁴⁰ but it is clear that parapsychologists are severely disadvantaged in terms of both financial rewards (for example, grants) and status; indeed, many young scientists thinking of a career in parapsychology are warned off by the elder statesmen of the field, to save them wasting their time.⁴¹ This is hardly plausible evidence of sufficient motivation to cheat! Until those believing in fraud can produce a satisfactory theory to explain its occurrence, we are unwise to accept their hypothesis as well founded.

The above arguments alone would seem to provide sufficient grounds for doubting the scientific status of the fraud hypothesis.⁴² Its failure to meet the very same canons of scientific rationality which, it is claimed, place the paranormal hypothesis outside of science raises the problem of why it has only been the latter which has been rejected for such reasons. As both parapsychology and fraud fare equally well (or badly) against demarcation arguments, it would seem that we need to explore just what more general role (if any) such arguments play in science.

THE ROLE OF DEMARCATION CRITERIA IN SCIENCE

The problem of demarcating 'pseudo-science' has usually been taken to be philosophical. For example, one of the best known attempts to solve the problem has been Popper's criterion of falsification.⁴³ Recently, however, the often idealized world of philosophical analysis has been juxtaposed with the actualities of scientific practice. The exploration of the resulting differences, largely initiated by Kuhn,⁴⁴ has been taken up in the work of those sociologists of science who are interested in the microprocesses of scientific activity.⁴⁵ Some of this work has been directed towards analyzing scientific controversies, and has shown that, in such episodes, the assumptions made by scientists as to what constitutes valid knowledge become visible. It has thus become possible to gain some understanding of how the problem of demarcation is solved in actual scientific practice, rather than in philosophical theory.

The few such case-studies to date can do no more than point to a possible sociological interpretation. The main work carried out has been Collins' studies of the criterion of repeatability,⁴⁶ which are supported to a certain extent by Wynne.⁴⁷ The general conclusion which emerges is that the repeatability of an experimental finding alone rarely produces an unambiguous 'yes or no' verdict on the scientific validity of a knowledge claim. It appears that this inconclusiveness is largely due to disagreement over what the particular criterion (replication) *means* in a certain context: the two disputing parties have differing notions of what constitutes a 'competent replication'. Apparently, scientists can produce endless reasons to label certain experimental replications as 'incompetent'. Lacking any agreed standard of 'competent replication', they cannot agree on applications of the criterion of repeatability. What

one side considers to be plausible conditions to produce a competent replication may be unacceptable to the other. The rejection of a claim because it is unrepeatable therefore rests on judgements of plausibility, which are inevitably culture-bound, and these cannot provide the necessary independent basis required of a universal standard of rationality.

The rejection of a belief because the evidence for it has not been replicated is, however, not unimportant — it serves to make *visible* the implausibility of the belief. That is to say, demarcation criteria (if we can generalize from the case of replication) do not appear to contribute to an explanation of why a particular belief is aberrant: they merely emphasize its aberrance. This does not mean, however, that demarcation criteria serve a solely legitimating function. They are, as Collins in particular has emphasized,⁴⁸ open to negotiation — as are the conditions required to produce a competent replication. Such negotiations may even help to decide the characteristics of the belief under examination, because they can lead to agreements over the conditions under which particular phenomena are manifest. The failure of a belief to be accepted as valid scientific knowledge lies in the claimant's inability to negotiate a satisfactory outcome. This lack of success can be understood as resulting from differences in meanings, definitions and tacit conventions (which may ultimately rest on different social interests). The point is that demarcation criteria, although not providing an independent guarantee of scientific rationality, do seem to offer a means whereby scientists can (at least in some circumstances) explore the difference between rival claims. Such claims are not rejected out of hand — there is often room for negotiation.

In the case of parapsychology's clash with orthodox science, however, there has been little room for negotiation; the cultural gulf between the two has been too great. For there to be a basis for the kind of negotiation which Collins observed amongst gravity wave scientists⁴⁹ and amongst parapsychologists⁵⁰ there must be enough shared assumptions, and this is not the case when the conflict occurs between the broad front of established science and beliefs on the periphery of science. As parapsychologists know, a blanket rejection is the most likely outcome in such cases. The incantation of demarcation criteria in order to reject parapsychology out of hand shows such criteria in their legitimating role. They gloss over the cultural differences to produce an apparently clear-

-cut case of 'non-science'. The more radical the claim, and the more it is separated from cultural orthodoxy, the easier it will be to establish its lack of scientific status.

This argument is in danger of becoming circular unless cultural marginality can be specified in some other way than simply by lack of scientific success. One thing which does appear to affect the centrality of knowledge claims is what their implications are for accepted knowledge. For instance, a claim will often be regarded as marginal if it involves a clash with present knowledge — perhaps leading to the overthrow of all or part of it. It seems that parapsychology is held to be culturally marginal for just this sort of reason. The cultural centrality of fraud, and the marginality of parapsychology, have been conveniently juxtaposed by G. R. Price when making the case for fraud in parapsychology:

If, then, parapsychology and modern science are incompatible, why not reject parapsychology? We know that the alternative hypothesis, that some men lie or deceive themselves, fits quite well within the framework of science. The choice is between believing in something 'truly revolutionary' and 'radically contradictory to contemporary thought' and believing in the occurrence of fraud and self delusion. Which is more reasonable?⁵¹

Clearly, the 'normal' hypothesis is, almost by definition, more central than the paranormal hypothesis: hence the weight of the demarcation process has been directed towards denying scientific status to the claims of the parapsychologists.

The astute reader will have noticed that Price's argument is none other than Hume's in a fresh garb. That scientists themselves use Hume's argument as a justification for rejecting parapsychology points to the perils of sociologists of knowledge embracing his approach. To use part of the scientific argument as the basis of our account is not to have explained anything at all.

CONCLUSION

In my view, demarcation criteria do not provide us with independent access to the scientific validity of beliefs. That such criteria seem to work equally well against both fraud and parapsychology indicates that they alone cannot account for parapsychology's re-

jection. Their use against parapsychology seems to have served primarily as a means to legitimate present orthodoxy. This, of course, is not to say that such arguments are not an appropriate way of handling the paranormal. This paper is pessimistic about the possibility of establishing independent standards of rationality: but this does not imply that we have good grounds for embracing *any* belief. It would thus be a mistake to read it as in any way supporting the claims of the parapsychologists. My interest is *not* in the *validity* of either hypothesis, but in the types of argument made for and against them. Indeed, it is a consequence of the position I am advocating (that demarcation arguments are culturally dependent) that the hypothetical arguments mounted against fraud in this paper will be unlikely to sound convincing in today's scientific climate.

Recently I had the opportunity to sound out attitudes when I presented my case to a group of scientists largely engaged in orthodox research work.⁵² One objection I received was that the one-way-mirror experiment was, in fact, good repeatable evidence of fraud because Pamplin and Collins had managed to repeat their own observations. I, in turn, argued that repeated observations by the same scientists did not constitute replication. A dialogue ensued. What, to me, was non-repeatable, was (at least to one contemporary scientist) a perfectly competent replication.

Thus, paradoxically, my fictitious arguments against fraud may not seem completely convincing to most scientists — apart possibly from some parapsychologists. It remains to be seen whether sociologists of knowledge, who can hope for the possibility of escaping from contemporary cultural orthodoxy, will also find the case plausible.

● NOTES

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1. C. Scott and P. Haskell, 'Fresh Light on the Shackleton Experiments?', *Proceedings of the Society for Psychical Research*, Vol. 56 (1974), 59.

2. H. Schmidt, 'Towards a Mathematical Theory of Psi', *Journal of the American Society for Psychical Research*, Vol. 69 (1975), 315.

3. The ghost materialized as the central issue of the public scientific debate over ESP with the publication of an article on fraud in parapsychology by G. R. Price: 'Science and the Supernatural', *Science*, Vol. 122 (26 August 1955), 359-67. In an Editorial on 'Extra Sensory Perception' (*Science*, Vol. 123 [6 January 1956], 7), Dael Wolffe claimed that: 'The central issue is whether or not the possibility of fraud has been ruled out'. See also C.E.M. Hansel, *ESP: A Scientific Evaluation* (New York: Charles Scribner's Sons, 1966), and C. Scott and P. Haskell, ' "Normal" Explanations of the Soal-Goldney Experiments in Extrasensory Perception', *Nature*, Vol. 245 (7 September 1973), 52-54. For a recent discussion of subject fraud, see P. Diaconis, 'Statistical Problems in ESP Research', *Science*, Vol. 201 (14 July 1978), 131-36.

4. J. B. Rhine, 'A New Case of Experimenter Unreliability', *Journal of Parapsychology*, Vol. 38 (1974), 215-25.

5. See, for example, J. Hanlon, 'Uri Geller and Science', *New Scientist* (17 October 1974), 170-85; P. Morrison, 'Uri Geller: International Pied Piper of the Credulous and other matters', *Scientific American* (February 1976), 134-35; M. Gardner, 'Magic and Paraphysics', *Technology Review*, Vol. 78 (June 1976), 43-51; and R. Hyman, 'Review of *The Geller Papers*', *The Zetetic*, Vol. 1 (Fall/Winter 1976), 73-80.

6. This case is discussed by B. Culliton, 'The Sloan-Kettering Affair (I): A Story Without a Hero', *Science*, Vol. 184 (16 May 1974), 644-50, and '(II): An Uneasy Resolution', *ibid.* (14 June 1974), 1154-57.

7. Hansel, *op. cit.* note 3, 129-49.

8. Some of the methodological criticisms are documented by C. Ransom, 'Recent Criticisms of Parapsychology: A Review', *Journal of the American Society for Psychical Research*, Vol. 65 (1970), 289-307.

9. See L. A. Selby-Bigge (ed.), *Enquiries by D. Hume* (Oxford: Clarendon Press, 1975). The Humean position is advocated by, for instance, Marcello Truzzi, former chairman of the Committee for the Scientific Investigation of Claims of the Paranormal, in *The Zetetic*, Vol. 1 (Fall/Winter 1976), 4.

10. Laurent Beauregard, in a recent article in which he attempts to apply Hume's argument in a rigorous manner, claims that the antecedent probability of the paranormal hypothesis and the likelihood of a 'normal' explanation for the evidence (which both have to be established before Hume's argument can be applied) can be assigned in a 'more or less objective way'. See L. Beauregard, 'Skepticism, Science and the Paranormal', *Zetetic Scholar*, Vol. 1 (1978), 9. However, by Beauregard's own admission, such assessments depend on 'the history of fraud in psychical research' (*ibid.*, 9) and are 'partly a matter of semantics' (*ibid.*, 9). It seems that such assessments are inherently dependent on our own cultural assumptions.

11. See T. J. Pinch and H. M. Collins, 'Is Anti-Science not-Science? The Case of Parapsychology', in H. Nowotny and H. Rose (eds), *Counter-Movements in the Sciences, Sociology of the Sciences*, Vol. III (Dordrecht: Reidel, 1979), 221-50. In this paper we discuss the compatibility issue in some depth.

12. This position is advocated by Price, *op. cit.* note 3, 361.

13. As one writer has put it: 'Much of the time we accept the ridiculous assumptions that if the investigator knows in his own heart that he is honest and objective, self-deception cannot occur.' C. W. Churchman, 'Perception and Deception', *Science*, Vol. 153 (2 September 1966), 1089.

14. One of the scientists working in the area considers that this particular piece of evidence finally clinches the fraud hypothesis.

15. H. M. Collins and T. J. Pinch, 'The Construction of the Paranormal', in Roy Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge, Sociological Review Monographs*, No. 27 (1979), 237-70. The prime concern of this paper is an investigation of the relationship between the content of arguments made for and against parapsychology and the context in which such arguments are presented. The fraud hypothesis is briefly discussed but there is no exhaustive examination of it, as in the present paper.

16. The revolutionary scenario is a device to sensitize the reader in order that the paranormal hypothesis is not rejected out of hand. It is not to be taken too literally, and all the arguments against fraud which are discussed are set in the present and depend on the state of the field *as it is now*.

17. J. C. Crumbaugh, 'A Scientific Critique of Parapsychology', *International Journal of Neuropsychiatry*, Vol. 2 (1966), 526.

18. This methodological principle is clearly stated by Hansel (op. cit. note 3, 236-37) as a condition which ESP experiments must meet.

19. In fairness, I should point out that there has, in fact, been one claim for this type of replication in the area of subject fraud. This was made by James Randi, a magician who specializes in emulating Geller metal-bending feats: J. Randi, *The Magic of Uri Geller* (New York: Ballantine, 1975), 256-63. Randi managed to convince the newspaper *Psychic News* (a popular spiritualist weekly) that he had produced phenomena which could be interpreted as paranormal — but which were, as he later revealed after publication, produced by fraud. *Psychic News* hardly provides the necessary stamp of scientific legitimacy.

20. Price, op. cit. note 3, 363.

21. Hansel, op. cit. note 3.

22. D. Marks and R. Kammann, 'The Nonpsychic Powers of Uri Geller', *The Zetetic*, Vol. 1 (Spring/Summer 1977), 9-17.

23. Scott and Haskell, op. cit. note 3. The difficulties of obtaining evidence of fraud is sometimes freely admitted — see, for instance, Diaconis, op. cit. note 3, 131.

24. See M. R. Barrington, 'Mrs Albert's Testimony: Observation or Inference?', *Proceedings of the Society for Psychical Research*, Vol. 56 (1974), 112-16, and I. Stevenson, 'The Credibility of Mrs Greta Albert's Testimony', *ibid.*, 117-29. In these papers, Scott's anecdotal evidence for fraud is challenged.

25. Hansel, op. cit. note 3, 181, makes this criticism of parapsychology.

26. See, for example, Hanlon, op. cit. note 5, and Gardner, op. cit. note 5.

27. See Hanlon, op. cit. note 5, 177, and 'Investigating Paranormal Claims', *New Humanist* (July/August 1976), 75, where attempts are described to get the famous fraud producer Uri Geller into the laboratory.

28. Marks and Kammann, op. cit. note 22, have recently claimed that they have replicated subject fraud by using 'naive subjects' (undergraduates) whose task was to attempt to reproduce a pre-drawn symbol, sealed in one (and, on some occasions, two) envelopes. They got significant results by this procedure which, they claimed, were of the same order of magnitude as those produced by Geller when attempting a similar task. However, the problem with this replication is, again, that it is difficult to attach any credence to the paranormal interpretation of the original experiment — which was conducted with Geller under informal conditions, and would be

rejected by most experimenters as totally inadequate. Unless it can be shown that the original experiment has a convincing paranormal interpretation, the experimenters do not appear to have replicated the important 'illusory' element of fraud.

29. B. Pamplin and H. M. Collins, 'Spoon Bending: An Experimental Approach', *Nature*, Vol. 257 (4 September 1975), 8. Gardner, op. cit. note 5, Morrison, op. cit. note 5, and Hyman, op. cit. note 5, all mention this experiment.

30. It is factors such as these which separate this experiment from other tests of deception employed by psychologists.

31. V. P. Zinchenko, A. N. Leont'ev, B. F. Lomov and A. R. Luria, 'Parapsychology: Fiction or Fact?', *Soviet Psychology*, No. 4 (1974), 15.

32. C. E. M. Hansel, 'Experiments on Telepathy in Children', *The British Journal of Statistical Psychology*, Vol. 13 (1960), 176.

33. Price, op. cit. note 3.

34. Hanlon, op. cit. note 5.

35. Hansel, op. cit. note 3, 237-38.

36. See, for example, the comments of Leon Harmon concerning the ease with which it is possible to invent 'normal' explanations for the 'remote viewing' experiments conducted at the Stanford Research Institute, in *Proceedings of the IEEE*, Vol. 64 (1976), 1259.

37. Scott and Haskell, op. cit. note 3.

38. The requirement that the fraud hypothesis should meet any criteria of simplicity is explicitly rejected by Hansel, op. cit. note 3, 17.

39. T. S. Szasz, 'A Critical Analysis of the Fundamental Concepts of Psychical Research', *Psychiatric Quarterly*, Vol. 31 (1957), 98.

40. Hansel, op. cit. note 3, 235.

41. See Collins and Pinch, op. cit. note 15.

42. I have restricted the above arguments to the type normally presented in the formal scientific literature from which our knowledge is constituted (the 'constitutive forum'). Of course, the case against fraud could have been made more meaty by using material normally reserved for the less formal 'contingent forum' of scientific debate (for an elaboration of this distinction see Collins and Pinch, op. cit. note 15).

43. K. R. Popper, *Conjectures and Refutations* (London: Routledge and Kegan Paul, 1963).

44. T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, 2nd edn, 1970). See also the debate between Kuhn and various philosophers of science in I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970). The relevance of this debate to the problem of demarcation has been stressed by D. L. Phillips, in his *Wittgenstein and Scientific Knowledge* (London: Macmillan, 1977), 142-68.

45. I am thinking here of some of the recent case studies of science carried out by Harry Collins, Brian Wynne and Steve Woolgar. See, especially, H. M. Collins, 'The Seven Sexes: a Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205-24; B. Wynne, 'C. G. Barkla and the J Phenomenon: A Case Study of the Treatment of Deviance in Physics', *Social Studies of Science*, Vol. 6 (1976) 307-48; S. W. Woolgar, 'Writing an Intellectual History of Scientific Development: The Use of Discovery Accounts', *ibid.*, 395-422.

46. H. M. Collins, 'Seven Sexes', op. cit. note 45, and 'Upon the Replication of Scientific Findings: A Discussion Illuminated by the Experiences of Researchers into Parapsychology', paper read to the joint conference of the Society for Social Studies of Science and the International Sociological Association, Cornell University, 6 November 1976.

47. Wynne, op. cit. note 45.

48. Wynne (op. cit. note 45) and Collins (op. cit. notes 45 and 46) appear to place slightly different emphases on repeatability in science. Wynne seems to view it mainly as a legitimacy device used to rationalize the rejection of beliefs *already rejected* for other reasons. Collins, on the other hand, stresses also the exploratory function served by demarcation criteria (such as repeatability), in that the reasons for rejecting the belief (such as the different cultural assumptions on which it is based) often become apparent *during* the negotiations over such criteria.

49. Collins, op. cit. note 45.

50. Collins, op. cit. note 46.

51. Price, op. cit. note 3, 361.

52. 'Symposium on the Analysis of Scientific Discourse', San Diego, The Salk Institute, 15 April 1977.

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